MONTHLY NOTICES

OF THE

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WILLIAM HUGGINS, ESQ., F.R.S., President, in the Chair.

C. J. Lambert, Esq., 1 Crosby Square, E.C., and
Arthur Schuster, Esq., Ph.D., St. John's College, Cambridge;

were balloted for and duly elected Fellows of the Society.

Mr. D. Gill's Expedition to Ascension. Report, November 11, 1877.

We returned from Green Mountain on October 16 and I resumed observations the same evening.

During the past month the weather has been most unfavourable. Had the opposition of *Mars* occurred in October the result would have been complete failure: only one series of morning observations would have been possible, though there were a good many clear evenings. I have chiefly been occupied with the triangulation of the *Mars* stars of comparison; indeed, when they were conveniently situated for observation I have devoted myself exclusively to this work. Three good nights' work will now finish it, and I hope to send a complete copy of the observations by next mail.

Some observations have been made by daylight of the distance and position-angle of the stars α_1 α_2 *Centauri*. The results show a very small increase in the position-angle, and a marked diminution of the distance (viz. from about $2''{\cdot}15$ to $1''{\cdot}75$) as compared with the results previously communicated. It is probable therefore that very soon the distance will be too small for measurement with so small an aperture (4 inches); as it is, observations are only possible in exceptionally favourable circumstances. I delay sending more detailed results till the series is complete.

In the Melbourne Catalogue there is a star, ϵ *Indi*, having the very remarkable annual proper motion of $4''{\cdot}2$.

As it happens, this star is favourably situated for observation when the *Mars* stars are in the zenith, and when they, for "break-neck" reasons, are almost unobservable. I have therefore measured the distance and position-angle of ε *Indi* relative to five neighbouring surrounding stars, and hope this may serve as the foundation at some future day of a determination of its parallax and proper motion. The distance of *Saturn* from *Mars* was measured on five days, symmetrically disposed with respect to the remarkable conjunction of November 3. The result may afford some interesting data as to the accuracy attainable by means of the heliometer in such measures, and give a determination of the relative positions of these planets which may be of value for future reference.

The proposed observations of *Melpomene* should begin on the 12th inst. (to-morrow), and I trust the weather will suddenly become more favourable.

Mars Bay, Ascension Island,
November 11, 1877.

Remarks accompanying Drawing of Mars. By Geo. D. Hirst, Esq.

As *Mars* during this opposition is not so favourably placed for observation in your latitude as he is here, I venture to forward the enclosed sketch in the hope that it may prove of interest. The drawing has been carefully executed from a fine $10\frac{1}{4}$ in. reflector, and represents very nearly the appearance of the planet at the present time.

Until lately the markings on *Mars* have been very indistinct, or almost invisible. Even on nights of the finest definition they have been too faint to attempt any effort at delineation, but within the last few days whatever it is that has interfered with our view, either in our own atmosphere or that of the planet, has removed, and the features I have endeavoured to depict are very distinct.

379 *George Street, Sydney, N. S. Wales,*
August 24, 1877.

The Physical Condition of Mars. By John Brett, Esq.

The following remarks are founded on a series of telescopic observations of the planet from August 2 to October 8, 1877, made with a 9-inch (With-Browning) reflector at the southern extremity of England near the Lizard signal station. The conditions were favourable, and no good opportunities of observation

were neglected; but the results I have to lay before the Society are in one sense extremely unsatisfactory, inasmuch as their tendency is to disturb, if not overthrow, conclusions which have hitherto met with general acceptance. The instrument employed, although of very moderate aperture, is good, and in vindication of its quality it will be sufficient to record that I have had a satisfactory view of the outer satellite of *Mars* with it (on the 15th of September) and, at an earlier date, of the satellite of *Neptune*.

As regards the disk of the planet *Mars*, I must call it emphatically a bad telescopic object, for, under the same conditions of altitude and steadiness, its image will not bear comparison for a moment with *Jupiter* in respect either of quantity or beauty of delicate detail; whilst *Saturn*, as compared with it, is immeasurably superior.

All the modern drawings of *Mars* exhibit well-defined half-tints pervading the so-called seas. I cannot give any account of any of these half-tints. There are certain parts of the dark spaces considerably darker than others, notably so the principal round spot in the equatoreal region, and the conspicuous peak next preceding it. But the fact of these markings passing nearly over the centre of the disk, where of course they are seen through a less depth of atmosphere, is sufficient to account for their superior intensity, for the round spot at its first appearance, some two or three seconds within the limb, is not above the average darkness of the markings.

This circumstance alone I consider sufficient to establish the fact that the planet's atmosphere possesses such a degree of opacity as to prevent the telescope doing itself justice on the details, and the total disappearance of these details two or three seconds within the limb of course removes all doubt about their being below the surface. The opacity of the atmosphere is further and most conclusively shown, as I think, by the great brightness of the disk at the limb. The contrast with *Jupiter* in this respect is most interesting.

It is not known that *Jupiter* has any solid nucleus, nor any atmosphere, strictly so called. We only know that he is semi-transparent and limpid to an enormous depth; and his brightness fades away sensibly towards the limb. *Mars*, on the contrary, is believed to be a solid body (his general topography being permanent), with a true atmosphere, and his brightness conspicuously increases towards the limb. The theory that the redness of the so-called land was owing to absorption in the planet's atmosphere is obviously untenable, for the limb, where the atmosphere alone is visible, is nearly white, whilst the central part of the disk is of a glowing red, very like any ordinary solid body heated to a dull red heat. That it is really red hot would of course be inconsistent with the received theory of the snows and seas, but there is reason to believe both these must be given up.

There is one salient fact about *Mars* which I fear cannot be

gainsaid, and if that is so, the hypothesis that his surface is habitable must be abandoned. There are no clouds.*

The past autumn has been singularly favourable for observation; I remember none equal to it.

Between the 2nd of August and the 8th of October there has been good definition on eighteen nights. There have been many more observing nights, but I have only counted those when it was worth while to draw. On no one occasion during this series of fine nights has there been a moment's doubt about the identification of the well-known features of the so-called seas. No one of them has been masked or disfigured beyond recognition. Now, supposing there to be land and sea, the evaporation and consequent cooling should be unequal in different regions, and currents must be generated, and therefore clouds wafted hither and thither; and the seas would thereby be sometimes hidden.

Terrestrial clouds are as opaque as anything in nature. The locality of the midday Sun cannot even be suspected when masked by an ordinary cloud, much less could the difference between land and sea be perceived.

That a whole hemisphere should be entirely exempt from clouds for more than two months continuously is, I submit, fatal to the seas.

No man will say that the atmosphere may not be dense enough to sustain clouds, for its density is obvious to the eye; besides, the so-called snows have to be accounted for. Between vapour and snow, the intermediate condition of cloud, or chilled vapour, is inevitable.

If any man says the body of *Mars* is so hot that the vapour will not condense in clouds, I have no objection to offer; but boiling seas are hardly satisfactory.

The white polar patch has always been regarded as snow; but there are one or two things to be said which are unfavourable to this hypothesis, besides the want of clouds to deposit it. In the first place the south polar patch now in view is surrounded with a dark stain, and this darkness is on one side continuous with the so-called sea, and of equal darkness with it. There is no break nor barrier in the straits which join the polar dark patch to the equatoreal dark patch. Therefore, if there be snow, it is lying on the sea, or on a polar island.

Beer and Mädler are said to have thought there was a marsh at the borders of the melting snow to account for this dark belt, but most of us would be unable to distinguish a marsh from a meadow when about a mile off. But the worst thing I have to say against the snow is that it lies far above the level of the solid body of the planet. Everybody knows that it appears to project beyond the limb in profile, but the irradiation of its greater brightness has been supposed to explain that away.

The *dark* details of the solid surface disappear long before

* A hypothetical exception is mentioned later.

they reach the limb, but the *white* detail sticks out beyond it; so that irradiation has a good deal to answer for.

Now I assert that the white patch is not continuous with the surface of the solid, but stands above it; and the evidence I adduce that such is the case consists simply in the shadow of the white patch, which could recently be seen to the eastward after the planet had passed its opposition. My first memorandum of this appearance is dated September 28 at 9^h, definition being on that occasion the very finest I have had this autumn.*

The first occasion on which the gibbosity of the disk was noticed was the 5th of October. It was then conspicuous.

The only hypothesis I offer respecting the white polar patch is that it consists of cloud: the neighbourhood of the pole being supposed to be the only region cool enough to condense the vapour.

There is no doubt that cloud would be bright enough, compact enough, and definite enough to supply all the observed peculiarities.

The Earth can show nothing whiter or more sharply defined than its cumulous clouds; and in *Mars*, if the temperature is so great as to dissipate them everywhere else, it is presumable that they would, even at the pole, form themselves only at the outer regions of the atmosphere.

This hypothesis accounts both for the apparent projection and the visible shadow.

The sum of my autumn's work upon *Mars*, therefore, is not only disappointing but grievous. The snows I am sure must be given up, and I sadly fear the seas must go too; and the one little companion globe in the whole solar system on which we could depend has its character taken away.

P.S. Since the 9th of October a patch of light has been noticed to the N.W. of the polar patch, of a diffused character, probably caused by specular reflexion from the outer surface of the envelope, since it is believed not to be affected by the rotation. This requires further observation.

November 9, 1877.

Notes on Mars, 1877. By H. Pratt, Esq.

Opportunities of observing the planet have not been numerous, for the weather here has often been indifferent. Yet on several occasions when definition has been really fine some of the markings have been very definite, and although the majority presented softened outlines, yet their complexity could only be realised by close and patient attention. On such occasions the number and variety of curved forms has been constantly remarked.

* It cannot be seen with ordinary definition.

In moments of the finest definition the markings have exhibited a *stippled* rather than a streaked character, and glimpses were obtained of a structure so complicated and delicate that the pencil cannot reproduce it, for the visual perception of it was too transient to be retained. Frequently what at first sight appeared as a broad hazy streak has been, by patient watching for the best moments, resolved into several separate masses of shading enclosing lighter portions full of very delicate markings. The spaces enclosed by the markings were sometimes of the general reddish tint, and occasionally almost white.

From the discrepancies that will occur between the most careful drawings of the same side of the globe made on different nights, and from the difficulty of the identification of the forms shown by the published drawings of previous observers, it has been difficult to believe that the perfect and *complete* form of any particular marking is ever seen on any one occasion. Hence the multiplication of careful drawings appears the only way to arrive at a fair knowledge of the features of the planet.

The persistence with which many well-known markings recur again and again during many years, as they have recurred from night to night during the present season, surely proves their connection with the globe itself rather than with its atmospheric envelope. And yet the difference in the details of any one form as seen with the same instrument, by the same observer, on different occasions, while our own atmosphere has been equally clear, as tested by reference to *Saturn*, surely points to variations in the opacity of the aerial envelope of *Mars* as the cause. Indeed, the idea of *local obscurations* as from a cloudy condition of the planet's atmosphere has seemed sufficient to account for many such discrepancies, though why certain forms should be visible at one time, and either *obliterated* or *greatly modified* at another time, it is not easy to decide. Drawings II. and VI. illustrate these difficulties.

The *exaggeration of the polar cap* by irradiation, which causes it at times apparently to extend beyond the planet's limb, has been observed a few times, but only on nights which, for other reasons, were deemed insufficiently good for any delicate observation. On the finest nights the polar cap has always appeared precisely as if it formed a portion of the actual surface of the globe.

On every occasion of really good definition the markings were most easily made out on the following limb, and often very closely approached it, as was remarked by Capt. Noble and others at the November meeting.

Although generally the atmospheric envelope of *Mars* has appeared clear by allowing the markings to be well seen, yet frequently the great difficulty of detecting any markings at all over large regions where markings undoubtedly do exist has seemed to indicate the existence of a partially opaque condition, although no well-defined cloud-masses like those observable on *Jupiter* were ever detected. That this effect was not caused by our own atmosphere has been proved by the superior definition

of *Saturn* at a lower altitude. This has occurred on different occasions, and is instanced on November 14, when, in the S.W. quadrant, the region bounded by the curved shading shown on Drawing VI. was of that character, and the whole region was whitish, the otherwise prevailing red tint being absent.

On every observing night this season the prevailing tint of the red portions of the planet has been much paler than during recent oppositions.

The general colour of the markings has appeared of a neutral gray, that of the most intensely marked portions inclining to bluish. Not the slightest trace of green has been detected.

The intense whiteness of the polar caps during August has gradually declined in purity of tint since then, becoming more nearly the same as the limb.

On all sides towards the limb the redness has always paled rapidly almost to white.

The six drawings accompanying these notes are selected from a series, and refer to nights when the best definition prevailed.

The longitudes appended are derived from Mr. Marth's Tables.

The instrument a Newtonian Equatoreal, speculum by With, of 8·15 in. aperture, diagonal prism of fine quality, power 400 single achromatic.

The Approximate Fraction Elements of Solar System.

By S. M. Drach, Esq.

In the weekly *English Mechanic* for June 16 and July 7, 1871, a writer signing himself G. F. H. gave remarkably close and simple approaches to the orbital mean distances and eccentricities, which I think ought to be specially recorded in the *Monthly Notices*. I have corrected the faulty 487 of *Saturn's* denominator, and extracted the factors:

Mercury	·3870981 − ·0000013 =	$\frac{12}{31}$;	
Venus	·7233316 +	17 = $\frac{217}{300}$, or	$\frac{7.31}{10.30}$;
Earth	1 −	00 = 1;	
Mars	1·5236923 −	15 = $\frac{611}{401}$, or	$\frac{13.47}{401}$;
Jupiter	5·2027760 +	18 = $\frac{1873}{360}$, or	$\frac{11.31.131}{6.27.53}$ true;
Saturn	9·5387861 −	30 = $\frac{4550}{477}$, or	$\frac{5.7.13}{9.53}$;
Uranus	19·1823900 −	01 = $\frac{4051}{159}$, or	$\frac{4051}{3.53}$;
Neptune	30·0566000 +	40 = $\frac{1593}{53}$, or	$\frac{27.59}{53}$.

Remark the 1, 3, 9, 27 times 53.

For eccentricities.

Mercury	·2054925 − ·0000001 =	$\frac{217}{1056}$,	or	$\frac{7.31}{32.33}$;
Venus	·0068722 +	07 = $\frac{2}{291}$,	or	$\frac{2}{3.97}$;
Earth	·0167918 −	08 = $\frac{9}{536}$,	or	$\frac{9}{8.67}$;
Mars	·0931125 −	03 = $\frac{73}{784}$,	or	$\frac{73}{28.28}$;
Jupiter	·0481626 −	04 = $\frac{38}{789}$,	or	$\frac{2.19}{3.263}$;
Saturn	·0561502 −	05 = $\frac{21}{374}$,	or	$\frac{21}{17.22}$;
Uranus	·0466683 −	17 = $\frac{7}{150}$,	or	$\frac{7}{10.15}$;
Neptune	·0084962 −	00 = $\frac{10}{1177}$,	or	$\frac{10}{11.107}$.

Nov. 1877.

On the Synodisms of Saturn's Satellites. By S. M. Drach, Esq.

In Mr. Christie's journal, *The Observatory* (No. 7, for last month), Mr. D. Kirkwood deduced from Lovnics' *Astronomy* (by adding $0^s{\cdot}62$ to period of *Mimas*) this interesting formula of the four innermost moons:

$$5(n_1 - n_2) + (n_3 - n_2) + 4(n_4 - n_2) = 0.$$

In this form I think it may lead to a theory akin to that arising from the relation to *Jupiter's* lunar $n_1 - 3n_3 + 2n_2 = 0$ between the mean motions of *Jupiter's* satellites. Recommended to try later elements for all, I have selected Guillemin's *Heavens* (English translation, 1876). Chambers' *Descriptive Astronomy* only gives the *minutes*; the columns of "days and decimals" could have been better replaced by the *seconds* of time. (*Saturn's* rotation, $10^h\ 29^m\ 17^s$).

		Period. d	h	m	s	In *Sat.* days.	Rev. in *Sat.* year.
I.	M.	0	22	37	23	2·157305	11414·08
II.	E.	1	08	53	07	3·135499	7852·185
III.	Te.	1	21	18	26	4·319888	5699·331
IV.	D.	2	17	41	09	6·262918	3931·155
V.	R.	4	12	25	11	10·33745	2381·678
VI.	Ti.	15	22	41	25	36·48820	675·2189
VII.	H.	22	07	07	41	51·02262	482·5411
VIII.	J.	79	07	54	40	181·5310	135·6271
	⊙		10759^d·22			24625·1	unity

Synodisms in Chronological Order.

Syn.	Terrestrial. d	h	m	s	*S.* days.
M.J.	0	22	13	42	2·18
M.H.	0	23	37	19	2·23
M.Ti.	1	00	02	40	2·30
M.R.	1	04	35	18	2·72
E.J.	1	09	47	46	3·19
M.D.	1	10	30	31	3·29
E.H	1	11	02	19	3·34
E.Ti.	1	11	58	37	3·43
M.Te.	1	21	11	07	4·25
Te.J.	1	22	59	43	4·42
Te.H.	2	01	29	53	4·71
Te.Ti.	2	03	23	29	4·87
D.J.	2	20	01	59	6·49
M.E.	3	00	26	43	6·91
D.H.	3	02	31	58	7·11

Syn.	Terrestrial. d	h	m	s	*S.* days.
E.R.	3	02	48	41	7·13
Te.R.	3	05	49	57	7·42
D.Ti.	3	07	17	47	7·56
E.D.	3	10	54	26	7·91
R.J.	4	18	58	17	10·96
E.Te.	4	23	56	39	11·44
R.H.	5	15	58	21	12·97
Te.D.	6	02	01	37	13·92
R.Ti.	6	10	36	42	14·42
D.R.	6	22	39	03	15·89
Ti.J.	19	22	57	41	45·68
H.J.	31	00	20	13	70·88
Ti.H.	55	23	25	22	128·12

⊙ 24625·1 *Sat.* days.

Fried. Weis published in 1860 *Gesetze der Satellitenbildung* (Perthes, Gotha, 8vo.). In the Library of the Royal Geographical Society.

Nov. 16, 1877.

On a New Astrophotometrical Method. By Prof. Ch. V. Zenger.

It occurred to me to measure the intensity of the light of planetary disks by the time they take appearing or disappearing in twilight, and I first tried it in April, when *Jupiter* was a morning star.

I was surprised at the beautiful regularity and the order in which the details of the planetary disk vanished, and the accordance of the determined intensity of light as well of the four satellites as of the details on the planetary disk.

It is obvious that the time of appearance and disappearance will be different according to the intrinsic luminosity of celestial objects, and that the object will vanish as soon as the heavenly background acquires by reflected light the same intensity as the observed star.

These observations are so easy, and the vanishing of the light is so obvious, that the method will give, even with untrained yet sufficiently sensitive eyes, good results, and will enable every possessor of a telescope from 2 inches aperture upwards to do with it useful work.

Most surprising is the result obtained by this photometrical method in the case of *Jupiter*, enabling us to judge of the relative brightness of the limb, of the planetary centre, of the different parts of the belts and polar zones, and lastly, of the brightness of the Jovian satellites.

It is a known fact, by the measurements of actinic and optic intensity of reflected sunlight from the atmosphere made by Prof. Bunsen and Roscoe, that both give parallel curves of increase and decrease at the same place of observation, and that the intensity is represented by the following formula :—

$$I = a + b \cos z - c \cos^2 z, \qquad (1)$$

I denoting intensity of reflected sunlight, a, b, c constants, and z the zenith distance of the Sun.

It is obvious that in our case z is very nearly 90°, and the term $e \cos^2 z$ very small, and we thus get approximately

$$I = a + b \cos z,$$

or

$$i = \frac{I}{a} = 1 + \frac{b}{a} \cos z = 1 + A \cos z, \qquad (2)$$

A denoting a new constant, that may be determined by supposing the intensity to become zero when the Sun is 18° below the horizon. We obtain

$$i = 1 + A \cos 108° = 1 - A \sin 18° = 0,$$

and therefore

$$A = 3{\cdot}2361 \qquad (3)$$

By a known formula we obtain

$$\cos z = \sin \delta \sin \phi + \cos \delta \cos \phi \cos t = \cos \phi \cos \delta (\cos t + \tan \phi \tan \delta),$$

or

$$I = 1 + 3{\cdot}2361 \cos \phi \cos \delta (\cos t + \tan \phi \tan \delta). \qquad (4)$$

Supposing the Sun's declination to be $\delta = 0$, and $t = 90° = 6^h$, we obtain, in equinoctial time, the Sun being in the horizon, $I = 1$, the unit for photometrical measurement.

Tab. 1.—It is from the above formula (4) I calculated a table to give, by interpolation, the brightness of twilight for Sun's declination $= +20°$ to $-20°$ and $t = 4^h$ to $t' = 8^h$.

Tab. 2.—The second table gives the observations of *Jupiter* April 1877 to end of October, containing time of appearance and disappearance of Jovian details and satellites with a 4-inch equatoreal, power 56 to get a large field.

Tab. 3.—It is obvious that the condition of the atmosphere will affect the absolute time of disappearance and appearance,

but not the relative time of it; I therefore computed the brightness relative to the third satellite, which gave the least variations of brightness of all in the Jovian system. The results are given in the third table.

The result is that the order of Jovian details, as shown by the means of observed brightness, will be as follows:—

First in brightness was observed the northern limb of the planet's disk, near the equator, then follow the middle part of the northern equatoreal belt, the northern polar zone, the southern belt (middle part of it), the southern polar zone; next to it comes the third satellite, its brightness differing only 5 per cent. from the brightness of the planetary disk near the southern pole, the second and first satellites both very nearly of the same brightness, yet sometimes interchanging in brightness, and finally the fourth satellite, the weakest in brightness, yet very variable, attaining sometimes nearly the brightness of the second satellite, yet decreasing to nearly one-half of the brightness of the third satellite.

The means, computed as shown above, are:

		Satellites.	
Limb of planetary disk	1·555		
Equatoreal belt, N.	1·130	Third	1·000
Polar zone, N.	1·124	Second	0·970
Equatoreal belt, S.	1·110	First	0·961
Polar zone, S.	1·091	Fourth	0·820

A period of change of brightness seems to be indicated with the fourth and second satellites, the period being as a mean for the former 16·6 days, for the second nearly two days, both in accordance with the time of revolution. The small number of observations, and, still more, the small amount of change in the first and third satellites did not allow of deducing periods of change of brightness for these. As *Venus* becomes now visible for the smallest telescopes during daylight, it would be interesting to apply this method of observation to the planetary details, so as to determine the influence of reflecting power, apparent breadth of illuminated disk, and distance, the time of appearance of light, if any there be, on the dark part of disk, etc. The method being so plain, and requiring no apparatus for photometric measurement, I supposed it of interest to any possessor of a telescope from 2 inches upwards.

Prague, Oct. 17, 1877.

TABLE I.

Hour Angle.			*Intensity of Twilight.* ⊙'s Declination.								*Cosines of Zenith Distances.*								
h	m	°	−20°	Δ1°	−10°	Δ1°	0°	Δ1°	+10°	Δ1°	+20°	−20°	Δ1°	−10°	Δ1°	0°	Δ1°	+10°	Δ1°	+20°
4	0	60 0	1·124		1·570		2·000		2·398		2·757	+0·040		−0·183		+0·321		0·449		+0·564
				0·045		0·043		0·040		0·036			0·014		0·014		0·013		0·011	
	40	70 0	0·850		1·258		1·680		2·087		2·458	−0·056		−0·083		0·219		0·349		0·468
				0·041		0·042		0·041		0·037			0·002		0·014		0·013		0·011	
5	20	80 0	0·504		0·926		1·345		1·753		2·141	−0·159		−0·024		0·111		0·242		0·366
				0·042		0·042		0·041		0·041			0·014		0·014		0·013		0·012	
6	0	90 0	0·183		0·586		1·000		1·414		1·815	−0·262		−0·133		0·000		0·133		0·262
				0·040		0·041		0·041		0·040			0·013		0·013		0·013		0·013	
	40	100 0	...		0·246		0·655		1·074		1·796	−0·365		−0·242		−0·111		0·024		0·159
						0·041		0·042		0·042			0·012		0·012		0·013		0·013	
7	20	110 0	...		...		0·318		0·745		1·175	−0·468		−0·349		−0·219		−0·082		0·056
								0·042		0·043			0·012		0·012		0·014		0·014	
	0	120 0	...		...		0·000		0·430		0·879	−0·564		−0·449		−0·321		−0·183		0·039
								0·043		0·045			0·012		0·012		0·014		0·014	

TABLE II.

Date.	The Satellites. (4)	(3)	(2)	(1)	Equatoreal Belts. South Limb.	South Middle.	North Limb.	North Middle.	Polar Zones. South.	North.	Limb of Disk.	Two Fixed Stars near *Jupiter's* Sat. (4).
	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
April 7	17 8·0	17 17·5	17 11·0	17 16·0	17 42·0	17 44·0	17 46·0	17 59·0	18 0·75	18 15·0	18 25·5	...
June 12	15 16·0	15 26·1	15 24·1	15 21·0	...	...	...	...	...	...	...	...
„ 14	15 20·8	15 30·1	15 23·75	15 26·7	...	...	...	...	...	...	...	...
Sept. 11	6 39·0	6 26·0	6 28·0	6 36·0	...	6 15·0	...	6 25·0	6 19·0	6 25·5	...	...
„ 28	6 30·0	5 47·0	6 0·25	5 48·25	...	5 42·0	...	5 42·0	5 54·0	5 45·0	...	6 43·0 F_1
„ 30	6 35·0	5 54·25	5 56·5	6 1·0	...	5 40·0	...	5 40·0	5 48·0	5 45·0	...	6 37·0 F_1
Oct. 6	5 55·5	5 42·25	5 44·5	5 43·5	...	5 28·0	...	5 28·0	5 32·5	5 31·0	...	6 15·0 F_2
„ 7	5 46·25	5 36·5	...	5 39·0	...	5 26·0	...	5 26·0	...	5 28·5	...	6 14·0 F_2
„ 14	5 58·5	5 28·0	...	5 34·5	...	5 17·0	...	5 15·0	5 27·0	5 21·0	...	6 2·0 F_2
„ 21	5 15·0	5 37·5	5 8·25	5 6·25	...	4 50·0	...	4 52·5	...	4 56·0	4 45	...
„ 27	5 5·3	4 56·2	4 57·4	4 59·0	...	5 53·5	...	4 53·0	5 4·5	4 50·3	...	...

TABLE III.

Equinoctial Twilight—1·000 at 6h.

April 7	0·805	0·891	0·800	0·877	1·102	1·120	1·138	1·206	1·280	1·408	1·502	...
June 12	0·751	0·870	0·848	0·811		...		...	...	...	...	...
„ 14	0·753	0·806	0·769	0·787		...		...	...	...	...	...
Sept. 11	0·816	0·933	0·925	0·846		1·032		1·032	0·938	1·014	...	...
„ 28	0·588	0·934	0·926	0·828		0·974		0·974	0·878	0·950	...	0·486 F_1
„ 30	0·538	0·900	0·881	0·834		1·052		1·052	0·977	1·005	...	0·481 F_1

TABLE III.—*continued.*

Date.	The Satellites. (4)	(3)	(2)	(1)	Equatoreal Belts. South Limb.	South Middle.	North Limb.	North Middle.	Polar Zones. South.	North.	Limb of Disk.	Two Fixed Stars near *Jupiter's* Sat. (4).
Oct. 6	0·769	0·835	0·825	0·817		0·949		0·949	0·913	0·925	...	0·573 F_2
,, 7	0·864	1·014	...	1·004		1·056		1·056	...	1·046	...	0·632 F_2
,, 14	0·884	0·937	...	0·929		1·027		1·043	1·027	1·043	...	0·654 F_2
,, 21	0·935	1·055	1·014	1·032		1·149		1·127	...	1·127	1·059	...
,, 27	0·941	1·023	0·996	0·979		1·027		1·033	1·029	1·033	...	...
Means:	0·785	0·914	0·880	0·791		1·043		1·053	0·962	1·063	1·280	0·484
Sat. (3) = 1·000:	0·855	1·000	0·939	0·862		1·141		1·152	1·053	1·163	1·401	0·620
April 7	0·91	1·000	0·93	0·99		1·25		1·32	1·43	1·43	1·86	...
June 12	0·85	1·000	0·97	0·93		...		...	...	...	...	...
,, 14	0·93	1·000	0·95	0·98		...		...	...	...	...	...
Sept. 11	0·88	1·000	0·99	0·91		1·10		1·10	1·00	1·08	...	...
,, 28	0·62	1·000	0·99	0·94		1·03		1·03	0·94	1·01	...	0·52
,, 30	0·60	1·000	0·97	0·92		1·17		1·17	1·08	1·16	...	0·53
Oct. 6	0·91	1·000	0·99	0·98		1·14		1·14	1·02	1·04	...	0·67
,, 7	0·85	1·000	...	0·99		1·04		1·04	...	1·03	...	0·62
,, 8	0·95	1·000	...	0·99		1·10		1·11	1·09	1·11	...	0·69
,, 21	0·89	1·000	0·96	0·98		1·09		1·07	...	1·07	1·25	...
,, 27	0·92	1·000	0·97	0·97		1·01		1·02	1·01	1·02	...	0·525
Means:	0·840	1·000	0·970	0·961		1·110		1·130	1·091	1·124	1·555	0·655

Eclipses of Jupiter's Satellites in 1875, *at Willet's Point, N.Y.H., U.S.A.* By W. H. Bixby, Lieut. U.S. Engineers.

(*Communicated by E. B. Knobel.*)

Sat.	Phase.	Date and time of Observation. Month. d h m s	Assumed Chron. Correction. h m s	Corrected Willet's Point time of Observation. Sidereal. h m s	Mean Solar. h m s	Corresponding Washington M.S. Time. h m s	Correction to Am. Ephemeris. m. s	Remarks.
		Mean Solar Chron. 596.						
I	Reap.	Apr. 20 08 29 54·20	02 24·37−	...	08 27 29·83	08 14 23·8	+01 41·8	*See* Note A.; Chron. Corr. obtained from 2 sets of 7 observations, each on ⊙.
II	Reap.	May 2 14 13 48·00	00 36·20−	...	14 13 11·8	14 00 05·8	−01 25·4	*See* Note B.; ditto ditto.
		Sidereal Chron. 217.						
I	Reap.	May 4 11 31 32·66	3 32 58·15+	15 04 30·81	12 13 50·4	12 00 44·0	+ 0 13·2	*See* Note C.; ditto ditto.
I	Reap.	May 13 08 28 59·00	3 33 32·07+	12 02 31·07	8 36 57·35	08 23 51·0	+ 0 46·0	Windy; instrument vibrating at observation; time obtained from 2 sets of 7 observations, each on ⊙.
		Sidereal Chron. 141.						
I	Reap.	May 27 16 20 22·00	25 44·7 +	16 46 06·7	12 24 43·8	12 11 37·4	+ 0 04·2	Time obtained from transit observations.

Note A.—Satellite I. is, at best, rather faint with this Zenith Telescope; in this particular observation the satellite first appeared as a bright protuberance on the edge of *Jupiter's* limb, and could not have been seen until it had reached its maximum brightness. (*See* Note C.)

Note B.—At time of observation I was very confident that the time of reappearance was noted and recorded correctly; it would have been difficult to have made a mistake of nearly a minute and a half in recording the time. I tried to get other observations on this satellite, but cloudy weather prevented.

Note C.—After recording the time of first appearance, note was made of the time which elapsed before the satellite appeared to be of maximum brightness; this interval, from first appearance, was about ($01^m\ 30^s$) one minute and thirty seconds.

Location of Willet's Point.—Longitude, $4^h\ 55^m\ 06^s{\cdot}04$ W. of Greenwich; Latitude, 40° 47′ 20″·52 N.

Instrument.—Zenith Telescope: Aperture = 2·7 inches; focal length = 45·0 inches.

Phenomena of Jupiter's Satellites, 1875–77.

By the Rev. S. J. Perry.

Owing to the loss of two assistants, one of whom was carried off by a rapid decline, it has been necessary during the last two years to confine our attention at this Observatory almost exclusively to the routine work which would suffer most by interruption. This, along with the exceptionally bad skies of the two last summers, will account for the fewness of the observations I am at present able to forward, but I send them in hopes that they may help to fill up some lacunæ in more ample records:—

Date.	Satellite.	Phenomenon.	G. M. T. h m s	Obs.	Remarks.
1875. Feb. 4	II	Tr. I. first contact	17 52 1·8	W.C.	Very unsteady, thin clouds.
		,, bisection	17 55 7·7	,,	,, ,,
		,, last contact	17 58 5·9	,,	,, ,,
Apr. 12	I	Tr. I. first contact	11 57 30·2	,,	
		,, bisection	12 0 51·2	,,	
		,, last contact	12 3 24·7	,,	
,, 13	I	Ec. D.	9 18 32·7	,,	Very close to disk; good.
	I	Occ. R. bisection	11 31 1·0	,,	
		,, last contact	11 34 34·5	,,	
1876. Mar. 16	II	Occ. R. bisection	17 46 1·7	,,	Poor, clouds passing.
		,, last contact	17 50 46·9	,,	
Apr. 7	I	Sh. I. bisection	13 2 56·4	,,	Very faint.
	I	Tr. I. first contact	13 42 7·4	,,	
		,, bisection	13 45 23·3	,,	
		,, last contact	13 48 0·4	,,	
	I	Sh. E. bisection	15 6 22·9	,,	
,, 26	III	Ec. D.	11 46 21·8	,,	Light fading 10^m earlier; good.
	III	Ec. R.	13 21 14·4	,,	Cloudy, fair.
May 3	III	Ec. D.	15 38 2·8	,,	
June 1	I	Sh. I. first contact	9 45 19·4	S.P.	
	I	Tr. E. bisection	11 26 37·7	W.C.	
		,, last contact	11 30 36·4	,,	
,, 25	I	Ec. R. first seen	9 20 56·3	S.P.	
		,, full light	9 22 2·0	,,	
July 2	I	Ec. R.	11 15 34·5	W.C.	
,, 17	I	Tr. I. first contact	8 53 59·8	S.P.	Strong daylight.
		,, bisection	8 55 56·5	,,	Definition good.
		,, last contact	8 58 50·0	,,	

Date. 1877.	Satellite.	Phenomenon.	G.M.T. h m s	Obs.	Remarks.
June 15	II	Ec. D.	10 32 3·6	W.C.	
	II	Occ. R. first contact	1 29 10·2	,,	
		,, bisection	1 30 58·7	,,	
		,, last contact	1 33 57·0	,,	

Stonyhurst Observatory.

Observation of a Meteor. By J. L. McCance, Esq.

(*Communicated by Mr. Ranyard.*)

In the February number of the *Monthly Notices*, there is, under the heading "Progress of Meteoric Astronomy during 1876," a list of remarkable meteors observed during that year. Seeing that such a record is kept, I suppose that a short account of a fine meteor seen here lately may possibly have some interest. It appeared on Friday the 17th August, about 8.13 P.M., and its apparent path seems to have been one of about 15 degrees in length, beginning near *β Piscium*, at about R.A. $22^h\ 55^m$; Decl. 2° N., and disappearing at about R.A. $23^h\ 50^m$; Decl. 6° N., near *ω* in that constellation. It was a large round ball of bluish-green colour, and was as bright as the moon at First Quarter. The duration of its visibility was 2 to 3 seconds. In the *Standard* of the next Monday (the 20th) the (I presume) same meteor is mentioned as seen in the neighbourhood of Mornington Crescent, N.W.

Rathfern, Rayner's Road, Putney Hill, S.W.
1877, August 27.

On Jupiter's Third Satellite. By J. Tebbutt, Esq.

As my Notes on the physical peculiarities of *Jupiter's* third satellite, published in the *Notices* for December 1873, called forth interesting papers in the *Astronomische Nachrichten* from Professor Alexander and Dr. Klein, it may perhaps be well to record the following subsequent observations made by me of the same satellite.

The satellite was watched on March 4, 1874, but the beginning of the dark phase could not be determined, as the transit began before the planet rose above the horizon. The satellite reached its nearest approximation to the centre of the disk of its primary about $9^h\ 3^m$, being then only a faint dusky spot and not near so conspicuous as in the transits of April and May of the previous year. It was still extremely faint and near the edge of the disk at $9^h\ 55^m$. The Moon was only 2° or 3° from the planet. It must be observed that the chord traversed by the satellite was much shorter than in 1873.

The satellite was again observed as a dusky spot on April 23, 1874. The last contact at ingress occurred about $6^h\ 55^m$, the definition being very bad. Six minutes and a half afterwards the satellite was still visible as a very faint luminous spot, but altogether disappeared by $7^h\ 9\frac{1}{2}^m$. At $7^h\ 37^m\ 44^s$, the definition having improved, it became visible as a faint dusky spot, and was soon very conspicuous. It was, however, not near so dark as in April and May 1873. It was seen with difficulty at $9^h\ 29\frac{1}{2}^m$ and became undistinguishable from the disk four minutes afterwards. The last contact at egress occurred about $9^h\ 53\frac{3}{4}^m$.

A transit of this satellite was again observed on July 18, 1874. The last contact at ingress occurred at $5^h\ 25\frac{1}{4}^m$. Occasionally between $5^h\ 56\frac{1}{4}^m$ and $6^h\ 4\frac{1}{2}^m$ I glimpsed a dusky spot near the estimated position of the satellite, but there was no doubt about its visibility at the latter time. The definition was excellent and the satellite soon became conspicuous, although it was projected on one of the faint belts of the planet. It was nearly as dark as the shadows in transit. At $8^h\ 10^m$ it was distinguished with difficulty and was invisible at $8^h\ 18\frac{1}{4}^m$. The bisection at egress took place about $8^h\ 28\frac{3}{4}^m$.

The transit of May 22, 1875, was watched without any trace of the dark phase being seen; indeed, from subsequent observations of transits when the apparent chord of the satellite's path was very short, there appears to be an absence of this phenomenon. This circumstance appears to favour the explanations of Dr. Klein in No. 2014 of the *Astronomische Nachrichten*.

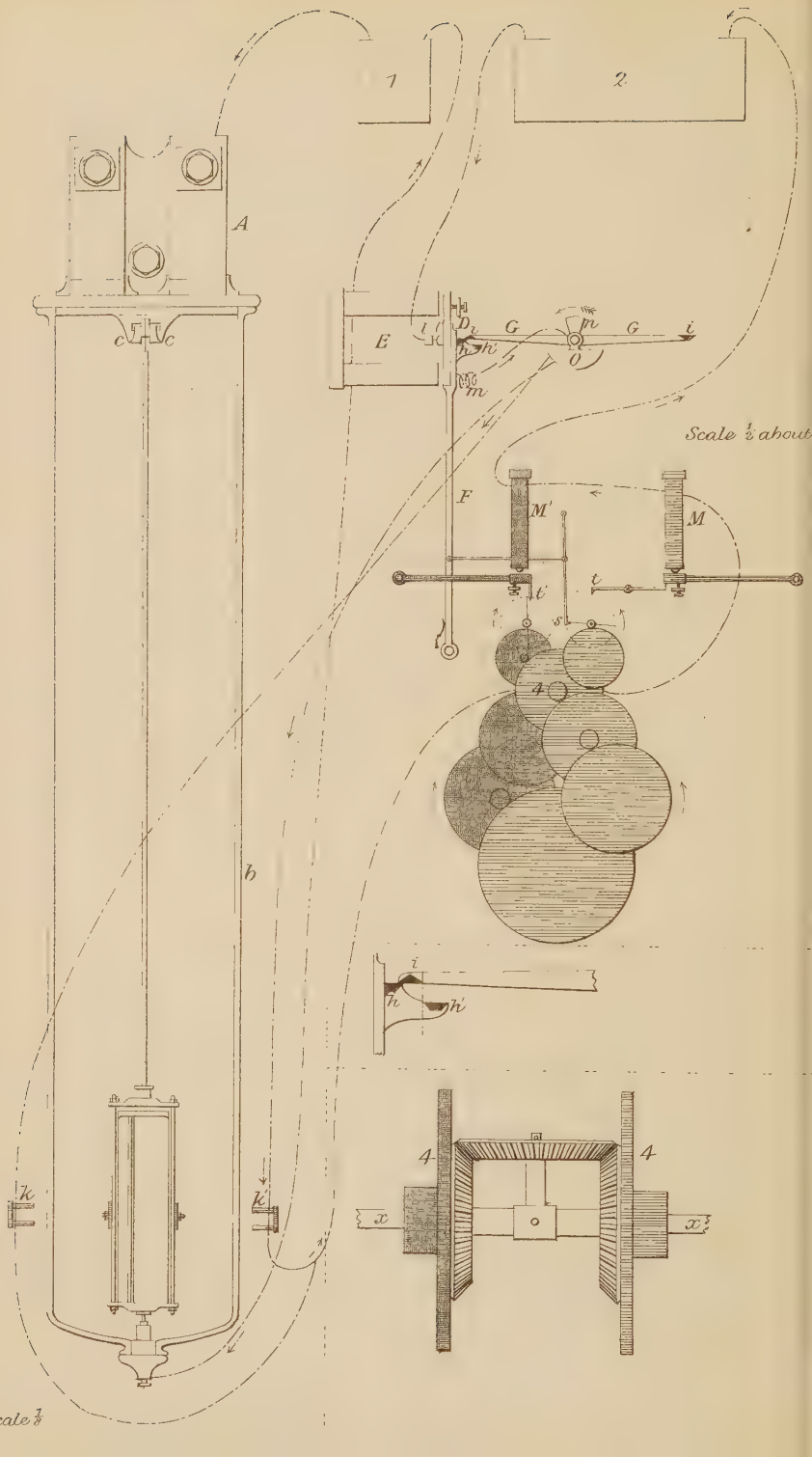
I may mention that the rosy colour of the equatoreal belts of the planet was beautifully seen on May 22, 1875, and May 5, 1877.

Windsor, N.S. Wales,
October 17, 1877.

A New Astronomical Clock.

By C. V. Boys, Esq., Associate of the Royal School of Mines.

In bringing before your notice this new form of Astronomical Clock it will be well first of all to describe the main principles on which it is founded, and then, by describing the details, to show how those principles are carried out. Of course on the pendulum everything depends. The drawing shows a mercury compensation pendulum supported by a spring, but whatever form is thought best might be used. As perfect compensation for atmospheric changes, though now practically almost perfect, can theoretically never be attained, it is an advantage to have the pendulum in a vacuum. This is good for another reason: it requires much less driving power to keep it swinging, and to changes in this power many of the irregularities of clocks are due. Mr. Carrington had a clock in a vacuum which was


1
2
A
c
c
E
D
G
m
G
i
h
h'
O
m
Scale ½ about
F
M'
M
t
t'
s
4
b
i
h
h'
4
4
x
x
k
k
Scale ⅛

wound up through a stuffing box; but this introduces a new error: the leakage at the stuffing box causes a slow increase of pressure inside the case, and this affects the uncorrected pendulum more than barometric changes in the air do a corrected one. Therefore, if the pendulum is in a vacuum, there must be no moving parts through the vacuum case. The impulse, which must, of course, be invariable, should be given to the pendulum while it is passing the middle of the swing; at all other times, especially at the ends of the oscillations, the pendulum should be absolutely free; also any work done by the pendulum, as unlocking pallets etc., should be at the middle of the swing. The impulse is usually given to the pendulum at a point about a quarter of the way from the top; but this is not the best place. An impulse, even if very powerful, given at the centre of oscillation would tend to turn the pendulum as a whole round its support; but if given anywhere else it would tend to bend the pendulum rod and to strain the supporting spring, and so would not act in the most perfect way. Lastly, a mechanical impulse cannot act so steadily as one due to an attractive force. In all these particulars the clock I am about to describe is as nearly as possible perfect; and, if what I have said is true, it should keep very accurate time.

I now come to the description of the clock itself. A is a powerful bracket secured to the wall; on the lower surface of this a ring is turned, to which the top of the glass case *bb* is secured. This case is afterwards to be exhausted. From the central projecting hooks *cc* hangs the pendulum, carrying at its lower end a small platinum disk, which, at the centre of each oscillation, touches a globule of mercury. 1 is a battery of low power, from which the current proceeds, first, to the supporting bracket, then down the pendulum rod to the mercury globule, round the electro-magnet E, and so back to the battery. Now it is evident that at the centre of each oscillation the electro-magnet E will attract its armature D. D is carried by an arm F, at the top of which is an escapement which the drawing sufficiently explains. The arm GG is driven by clockwork (not shown in the diagram) so that one of the triangular pins *i*, *i'* rests on one of the pallets *h*, *h'*. From what has been said it will be seen that at every swing of the pendulum the arm G will turn through 180°, that is, will turn 30 times in a minute; thus hands moved by the wheelwork driving the arm may be made to show hours, minutes, and seconds.

The impulse is given to the pendulum from outside the vacuum case in this way:—On each side of the pendulum is fixed a small piece of soft iron by a bolt in a line with the centre of oscillation of the pendulum. Outside the glass case are placed two small electro-magnets *k*, *k'*; if these can be alternately made magnetic the pendulum will be kept swinging. 2 is a battery as constant as possible, from which the current passes, first to the platinum block *l*, then to the platinum hammer *l'*, down the arm F, through the spring *m*, and on to the axis of the arm G. On this are

fixed two platinum sectors n, n', side by side but in opposite directions. Below there are a pair of mercury troughs o. When the arm G is horizontal one of the platinum sectors is in its trough while the other is out, so, as the arm G turns round, each trough is alternately put in connection with battery 2. Each trough is connected by a wire with one of the electro-magnets k,k'; after passing them the two wires join, pass through the regulating mechanism, and back to the battery. Owing to the platinum pieces l, l' being in contact only when the armature D is attracted, that is, when the pendulum is passing its vertical position, it follows that the pendulum will receive its impulse only at this time; also the impulse will act in a line passing through the centre of oscillation of the pendulum. So far therefore the pendulum is under the conditions stated at first, namely, it swings in a vacuum and so is not affected by atmospheric changes, and it requires a smaller impulse to keep it swinging; there is no moving part through the vacuum case, which can therefore be made perfectly airtight. The impulse is given at the middle of each swing of the pendulum. The impulse is in a line with the centre of oscillation. The pendulum does work, in this case touches the mercury, only in the middle of the swing. The impulse is not given mechanically, but by the uniform action of an electro-magnet. There is one point, however, of the greatest importance not mentioned yet; it is how to keep the impulse, and therefore the current from the battery 2, constant. If at one time the impulse is greater than at another, the pendulum will swing through a larger arc and will therefore go more slowly; but if by any means the impulse can be kept constant, the pendulum will likewise swing through a constant arc. To keep a continuous current constant is an unsolved problem; but the current we have to deal with is intermittent, and though it cannot be kept absolutely constant, yet its practical equivalent can be attained, namely, that the current shall never vary beyond two limits, which limits can be brought as near together as we please. The method of electro-magnet and armature fails to keep a continuous current constant for this reason: the armature, in order to work the regulating machinery, must have a certain amount of play between its stops, and therefore, when at its nearest a decidedly weaker current will be sufficient to keep it attached than would be necessary to attract it when at its greatest distance; therefore fluctuations of a similar amount will take place in the strength of the current. But in an intermittent current the case is quite different: every time the current starts the armature is at the same distance from the electro-magnet, and the same strength of current therefore will be sufficient to attract it every time, but if the current is ever so little weaker the armature will not be attracted. Now, suppose there are two electro-magnets in the same circuit, one of which will for a given strength of current just attract its armature, while the other will just not; then, if the current is of the right strength, one armature only will be attracted, if too weak neither,

if too strong both. These movements of the two armatures may be employed to turn a rheostat, through which the current passes, and so increase or diminish the total resistance according as the current is too strong or too weak. In the drawing, part of the regulating mechanism is coloured blue and part pink* ; the blue part is to regulate the current when too weak, and the pink part when too strong. M, M′ are the two electro-magnets on which the regulating action depends, so arranged that, when the current is right, the blue one only attracts its armature, when too weak neither, when too strong both. The effect of either electro-magnet on its armature depends on the dead weight of the latter, which is invariable, and on the distance, which can be regulated by a screw stop. This is better than using a spring, the strength of which changes with the temperature. This was suggested by my friend Mr. Liveing. Below these electro-magnets are two similar trains of wheels terminating in flies, and as they are to go in opposite directions, the same spring will drive them both.

Wheels 4 of each train run loose on the same axis x, but each carries a bevel wheel gearing with a third, as shown in the lower diagram, so either train is free to run while the other is still, but in so doing it carries the axis x round with half the velocity of its fourth wheel, and in the same direction, according to the formula $e = \frac{n-a}{m-a}$. The axis x turns the rheostat. The parts undescribed will be easily understood from the drawing. Suppose the current of the right strength: then, the moment the pendulum touches the mercury globule, D is attracted and the detent s is drawn back, but at the same time the electro-magnet M attracts its armature and the detent t prevents the blue train from running. The other electro-magnet only attracts its armature when the current is too strong, therefore the detent t' prevents the pink train from running, so both trains of wheels, and therefore the rheostat, keep still. Next suppose the current too weak. As before, the detent t' will be down; but the electro-magnet M, being unable to attract its armature, will leave the blue train free to revolve, so the rheostat will turn a little and diminish the resistance ; but if the current is too strong both magnets will attract their armatures, the blue train will be stopped, and the pink one will be free ; but it turns in the opposite direction to the blue train, therefore the rheostat will turn a little the other way and increase the resistance. If by any accident m' only were to act, both trains would be set running with an equal velocity in opposite directions and the rheostat would stand still. A single electro-magnet might be used instead of two, arranged to stop the blue train when attracted and the pink one when not attracted. This would keep the strength of the current oscillating on either side of the proper strength. There is no objection to the mercury arrangement $n\,o$, as when the circuit is made and broken the plate is fully

* The blue part is shown by faint shading, the pink part by dark shading.—Ed.

immersed, so there will be no sparks and the resistance will be constant. It is important that the two wires from the mercury troughs *o*, to the electro-magnets *k*, *k'* and on to the junction should have the same resistance, for if not it would be impossible to get a uniform current from the battery 2. Nearly if not quite all the objections to the mercury contact at the lower end of the pendulum are here done away with. The battery 1 may be of very low electro-motive force; the circuit is short, so that there will be hardly any self-induction to produce sparks. The mercury globule being in a vacuum, and if thought well in a hydrogen vacuum, its surface cannot get oxidised. As to the shape of the globule, a circular one, that is, one with a circular horizontal section, would not be good, as, if by chance the pendulum should swing not always in the same plane, but sometimes nearer the front and sometimes nearer the back, the duration of contact would not be uniform, therefore the duration of impulse would vary, which would be fatal. A long globule, then, with parallel sides would be free from this objection.

There is one more point of importance, which is the regulation of the clock. The pendulum would be made as nearly right as possible by the ordinary methods, after which the final regulation could be obtained with the greatest nicety without touching or interfering with the pendulum in any way. As a pendulum swings more slowly in a large arc than in a small, it follows that, by putting the electro-magnets *k*, *k'* nearer to the pendulum, and so increasing their action on it, it would swing through a larger arc and therefore more slowly; and in the same way, by putting them further off, the pendulum would go faster. The two magnets *k*, *k'* would be capable of being moved towards or from the pendulum according as the clock gained or lost. Lastly, the movement of the arm F could be made to send any number of currents every second without in any way interfering with the clock.

Ninth Catalogue of New Double Stars discovered with the 6-inch Refractor. By S. W. Burnham.

The following list comprises the new pairs found with the 6-inch Clark Refractor in the latter part of 1876 and the first part of 1877. During this period most of the time was devoted to the examination of known double stars, and but little attention given to the discovery of new objects. Many of these have been since observed by Baron Dembowski and Professor A. Hall, and their measures are given here.

Since July of the present year this series of double star observations has been continued with the 18·5-inch Clark Refractor of the Dearborn Observatory, and it is hoped that in some respects at least the results obtained have a proportionately increased value.

No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Pos. °	Dist. ″	Mags.
453	Arg. (56°) 338	1 37 8	+56 31	218·7	1·· ±	9·0... 9·5
454	O² Arg. 8280	8 10 12	−30 29	18·6	2· ±	8·0...10·0
455	L 18231	9 8 33	+ 4 43	65·2	1·94	9·5...10·5
456	L 22020	11 30 44	−11 41	68·2	0·65	9·0... 9·0
457	O¹ Arg. 11836	11 55 15	−20 51	84·3	0·89	8·0... 9·0
458	L 22677	11 58 8	−20 22	240· ±	20· ±	8·0...10·0
459	W¹ XII. 689	12 41 58	+ 4 7	285·2	3·61	8·5...11·0
460	W¹ XIII. 273	13 18 40	−15 0	215·2	2·07	8·0...10·5
461	W¹ XIII. 850	13 50 36	+ 3 34	220· ±	20· ±	7·5...11·0
462	Lam 1702	14 23 46	− 3 11	324·4	2·01	9·5... 9·8
463	...	18 10 46	−16 55	...	1·3 ±	9·0... 9·3
464	W¹ XVIII. 476	18 21 45	+ 6 29	111·3	1·20	8·5... 9·5
465	Arg. (56°) 2130	18 41 38	+56 45	293·2	3·06	9·0...11·0
466	W¹ XVIII. 1503	18 59 34	+10 39	167·7	1·64	9·0...10·0
467	O¹ Arg. 19936	19 39 24	−21 49	140· ±	1·8 ±	8·0...10·5
468	L 37571	19 39 58	+ 3 57	182·4	9·55	7·0...11·3
469	W² XIX. 1757	19 54 28	+24 24	175·4	14·42	8·3...10·6
470	O² Arg. 20079	20 3 41	+63 25	213·8	2·32	9·5...11·0
471	Arg. (61°) 2046	20 41 1	+62 0	125·9	1·46	10·0...10·0
472	Arg. (61°) 2078	20 57 12	+61 23	6·5	0·66	8·3... 8·5
473	...	21 1 26	−10 42	115·4	1·70	9·0...10·0
474	O² Arg. 23373	22 1 2	+60 25	360· ±	10· ±	8·5...11·8
475	L 43305	22 6 15	− 8 36	240· ±	1·5 ±	7·5...11·0
476	W² XXII. 180	22 8 41	+30 48	93·1	2·62	9·5...10·0
477	W² XXII. 225	22 10 28	+30 49	45·9	6·42	9·2...11·0
478	Lam. 4578 A and B	22 23 8	− 7 56	32·6	1·26	10·0...10·0
	A and C			239·0	28·45	10·0
479	Arg. (67°) 1444	22 26 28	+67 33	29·8	2·42	9·7...11·2
480	W¹ XXII. 716	22 35 18	+ 4 6	63·1	0·79	9·5...10·0
481	W¹ XXII. 1162	22 56 23	−11 53	53·0	1·31	9·0... 9·5
482	Arg. (62°) 2350 A and B	23 55 44	+62 39	360· ±	4· ±	8·5...10·0
	A and C			150· ±	10· ±	11·0

Notes.

453. A very difficult pair, the distance of which was at first estimated 1″·2. Baron Dembowski could not see it double, and upon a re-examination with the 6-in. on two or three good nights I was unable to see any certain trace of duplicity. Subsequently I tried it with the 18½-in. of the Dearborn Observatory, and obtained a fair measure of the position-angle. I found the distance much overrated, and the esti-

mate given above is probably still too large. It was a difficult object even with the large aperture, and impossible to measure in distance on this occasion. When discovered the entry made no mention of its being a particularly difficult pair.

454. Position-angle measured with the 6-in.

455. Measures by Prof. A. Hall (*Astr. Nach.* 2147), with the 26-in. (1877·3), on two nights. In Lalande this star is $7\frac{1}{2}$ magnitude. When found it appeared to be about 8·5 mag.

456. Excessively difficult; the most northerly of two small stars in the same field. Measures by Prof. A. Hall (*Astr. Nach.* 2147), with the 26-in. (1877·3), on two nights.

457. Measures by Prof. A. Hall (*Astr. Nach.* 2147), on two nights, with the 26-in.

458. Noted in observing the preceding pair.

459. Measures by Baron Dembowski (1877·5); two observations.

460. Measures by Baron Dembowski (1877·4); two observations.

461. Very minute distant companion.

462. A pretty but faint pair, directly south of Σ 1852 *rej.* Measured on two nights by Baron Dembowski (1877·5).

463. Not in any star catalogue I have. Place with micrometer from H 2824.

464. A third star, 11 mag., 90° : 25″. Measures by Baron Dembowski; two nights (1877·6). In a low-power field with OΣ 350.

465. Measured once by Baron Dembowski (1876·8).

466. Measured once by Baron Dembowski (1876·7).

468. Found in looking for OΣ 381. Observed three times by Baron Dembowski (1876·9).

469. A mean of three observations by Baron Dembowski (1877·0).

470. In the field with Σ 2642. Measures by Baron Dembowski; one night (1876·8).

471. A pretty little pair; found in looking for H 3000. In Argelander 9·4 magnitude. Measured once by Baron Dembowski (1876·8).

472. The wide pair in the field, south, is H 1607. Measures by Baron Dembowski; two nights (1877·2).

473. Not found in any star catalogue I have. A mean of two measures by Baron Dembowski (1876·7).

475. Very unequal and difficult pair.

476. This and the next in the same low-power field. Both measured by Baron Dembowski on two nights (1876·8).

478. Measured once with the $18\frac{1}{2}$-in (1877·8). Very difficult with the 6-in. There is a fourth smaller star, about the same distance as C in the opposite quadrant.

479. This makes, with two 8–9 magnitude stars, all in the field, a small equilateral triangle. A difficult object. Measured once by Baron Dembowski (1876·8).

480. Very difficult. Measures by Baron Dembowski; one night (1876·9).

481. Very difficult pair in the field with Σ 2970. Measures, one night, with the $18\frac{1}{2}$-in. (1877·8).

482. For the close pair Baron Dembowski, by two observations, gives 342°·7 : 4″·07 : 10·0...11·0 (1877·2). The principal star in Argelander is 8–9 magnitude.

Chicago, Nov. 5, 1877.

Extract from a Letter from the Rev. F. Howlett, to W. T. Lynn, dated 1877, November 3.

By this post I forward you careful drawings of the fine solar spot (now on the disk) for October 31 and November 1, taken by projection, power 120, and screen 5 ft. 2 in. from eye-piece, when each half-inch subtends just 15″. The main spot must have been very near the solar equator, especially when we consider that the Sun's north pole is now within the margin of the disk.

The lesser group "followed" the principal one after an interval of about 4′ 30″.

I would add that a friend who was watching me as I took the drawings noticed, as well as did I, a very distinct *haze* of a kind of bistre-brown colour extending for some 10″ or 12″ upon the penumbra, beyond the margin of the umbra, on October 31, as well also as a considerable sized nucleus of about 12″ diameter within the umbra.

The definition on October 31 was very fine, and it was very good also on November 1, though I could barely distinguish any of the bistre-brown haze or the nucleus on the latter day.

The "bridge," extending more than half-way across the umbra, was not very luminous on October 31, and still less so on November 1, and on each day aforesaid terminated in very faint flocculent matter.

To-day (November 3), this flocculent matter is very much feebler still, and is all that remains of the "bridge."

Note on a Special Case of "the Most Probable Result" of a Number of Observations. By J. M. Wilson, Esq., Rugby.

It is beyond the reach of all but pretty good mathematicians to follow the reasoning about "probable errors" and "the most probable results" of a number of discrepant observations. But there is one law of weighting observations which lends itself so easily to calculation that perhaps some of the non-mathematical readers of the *Notices* may be interested in seeing it. The result is not new, though it was quite new to me when I first observed it, some six or seven years ago, and I do not remember to have seen it since, except in Mr. J. W. L. Glaisher's paper in the *Memoirs* for 1872.

A number of numerical readings are made of some magnitude that is to be measured; they differ from one another. What is the most probable result? The common method is to take their arithmetical mean; and if the readings are tolerably evenly distributed this will not give a bad result. But if one reading is widely different from the others, the doubt occurs whether it ought to be wholly rejected or have less weight assigned to it.

On the principle of the arithmetical mean there is no choice; the observation must be accepted wholly or rejected wholly, unless of course arbitrary weights are assigned to the observations by an *ex post facto* judgment of them.

But suppose we agree that observations shall have less weight the remoter they are from the final result; in fact that the weight of an observation shall vary inversely as its error from the result; so that, if our final result is 27·4, an observation 27·5 shall have more weight than an observation 27·9 in the ratio of 5 to 1, and more than an observation 28·4 in the ratio of 10 to 1.

The most probable result of a number of readings will, according to this law of weighting observations, be obtained as follows:—

Let the observations be arranged in order of magnitude; *then, if the number of them is odd the middle observation must be taken; and, if the number is even, anywhere between the two middle observations must be taken, supposing all to be unequal.*

For the case is this. Let a rod without weight have a number of particles resting on it having weights each inversely proportional to their distances from the point of suspension. Find the point on which it will balance.

If the distance of any particle from the point of suspension is x and its weight $\frac{1}{x}$, then its moment is 1, and all that is necessary is that there shall be an equal number of particles on both sides of the fulcrum.

Hence if a number of readings of the distance, we will suppose of a double star, are taken as follows: 27·3, 29, 28·3, 26, 26·8, 27·1, 28, 28·5, 27·4, they must first be arranged in order of magnitude, 29, 28·5, 28·3, 28, 27·4, 27·3, 27·1, 26·8, 26; and, the number of them being odd, the middle one, 27·4, is the most probable result. The arithmetical mean is 27·6.

If the last observation were cancelled, the most probable result would be anywhere between 28 and 27·4; the arithmetical mean is 27·8.

There is one curious case in which this method gives apparently *no* most probable result. This case would have arisen if 27·3 is substituted for 27·4. The most probable result cannot be 27·3, for there then would be three readings on one side of it and four on the other; nor can it be more than 27·3, for then there would be five readings on one side and four on the other. Of course the solution of the paradox is to be found by supposing the two equal readings to differ by an infinitesimal amount, and the real result would therefore be 27·3.

Rugby, Nov. 1877.

Note upon the Determination of the Longitudes of Kurrachee and Madras. By Major-Gen. Thomas Addison, C.B.

In 1874 arrangements were made for exchange of galvanic signals for difference of longitude between the German Observers of the Transit of *Venus* at Ispahan and my station at Kurrachee, and also between the latter and the Madras Observatory.

Between the two former stations 225 signals were exchanged on the 11th and 12th December, and on the 13th 200 between Madras and Kurrachee.

Professor Auwers has favoured me with the results of the first sets, and Mr. Pogson did the same in the case of those with him. They are

Difference of Longitude.

	h	m	s
Ispahan—Kurrachee	1	1	13·09
Kurrachee—Madras	0	53	6·22

Dr. Becker, of the Royal Observatory, Berlin, has further kindly communicated to me the difference of longitude, similarly obtained, between Berlin and Ispahan; and as that between Greenwich and Berlin is given in the *Nautical Almanac*, the whole chain is thus completed from Greenwich to Madras. It stands thus.

		h	m	s
Berlin, east of Greenwich	By *Nautical Almanac*	0	53	34·9
Ispahan, east of Berlin	By galvanic signals by Dr. Becker and Dr. Fritsch	2	33	5·44
Kurrachee, east of Ispahan	By galvanic signals between Drs. Becker and Fritsch and General Addison	1	1	13·09
Madras, east of Kurrachee	By galvanic signals between Mr. Pogson and General Addison	0	53	6·22
Madras, east of Greenwich		5	20	59·65

Both Professor Auwers and Mr. Pogson expressed themselves well satisfied regarding the reliableness of the signalling operations, and I feel every confidence in the final result as concerns Madras.

That for Kurrachee has an element of doubt in it, inasmuch as I neglected to obtain the pivot error of my Transit Instrument. But to whatever extent the pivot error affected the result as regards Kurrachee, the difference between Ispahan and Madras remains unaffected, the errors on the east and west sides of Kurrachee correcting each other.

In the *Monthly Notices* of the Royal Astronomical Society for March 1872 are recorded the results obtained by Colonel Walker and Major St. John by telegraph between Teheran and Greenwich, from which was deduced the longitude of Teheran, viz.

$$51^\circ\ 24'\ 26''\quad \text{or}\quad 3^h\ 25^m\ 39^s{\cdot}73.$$

Major St. John had previously obtained the difference Teheran—Kurrachee. He does not give it, but he states that these two operations, combined with those of the great Trigonometrical Survey of India, bring out the longitude of Madras within half a minute of arc of that given in the *Nautical Almanac*.

The latter is $5^h\ 20^m\ 57^s{\cdot}3$; therefore he must have made the difference Teheran—Kurrachee $1^h\ 55^m\ 17^s{\cdot}57 \pm 2^s$.

In 1874 Dr. Becker determined the difference between Teheran and Ispahan, and this affords us the means of comparing the operations of 1872 and those of 1874.

According to Dr. Becker the difference Teheran—Ispahan is $58^s{\cdot}8$, and as Ispahan—Madras is $1^h\ 54^m\ 19^s{\cdot}3$, there results for Teheran—Kurrachee, by the operations of 1874, $1^h\ 55^m\ 18^s{\cdot}11$, from which Major St. John differs by $\cdot 54^s \pm 2^s$. The local time was obtained by Colonel Walker, by Sextant, that by Dr. Becker by Transit Instrument.

The longitude of Teheran obtained by Colonel Walker does not quite agree with that obtained by Dr. Becker. The latter was

	h	m	s
Berlin—Teheran	2	32	6·68
Long. Berlin by *Nautical Almanac*	0	53	34·9
Resulting longitude of Teheran	3	25	41·58,

from which Colonel Walker differs by $1^s{\cdot}85$.

At Kurrachee my Observatory was $0^s{\cdot}6$ east of that used in the great Trigonometrical Survey of India at that place. Major St. John's exact station is not given.

The Hill House, Melton, Suffolk,
December 4, 1877.

Extract from a Letter from R. J. Ellery, Esq., to the Astronomer Royal, dated Melbourne, 1877, *October* 3.

My Dear Sir George,—Your telegram of the 17th August relative to suspected satellites of *Mars* reached me on the 22nd, and we have taken every opportunity since of searching, but as yet without success. Unfortunately the great telescope has been *hors de combat*, part of the declination movement having broken. This is now nearly repaired, and in the meantime the

watch has been kept with the 8″ refractor. Our scrutiny assumes the satellites (if they exist) to be very faint objects, and we are obliged to eclipse *Mars* itself to enable us to take our search close to his limb.

Extract from a Letter from R. J. Ellery, Esq., to the Astronomer Royal, dated Observatory, 1877, October 31.

My Dear Sir George,—We continued our search for *Mars'* satellites up to the 27th instant, but, with the exception of one occasion, upon which *we believe* we saw one of them, our search has been fruitless. The occasion referred to was on the night of the 16th October, when *Mars* occulted a star of about the 13th magnitude at $22^h\ 15^m$ (Sid. Time); after emergence a very faint point (20th magnitude estimated) was seen half a diameter from *Mars* s.p.; this was watched for nearly an hour, when its position indicated a motion with *Mars*, but before any verifying measures could be made the sky became cloudy, and no other signs of satellites have been observed since. Our first search with the great telescope was on the 26th September, and it is very probable our want of success is due to our lateness in the field.

We have obtained a fine series of observations for the parallax of *Mars* with our Transit Circle.

The Opposition of Mars, 1877. By Maxwell Hall, Esq.

(*Abstract.*)

Observations of *Mars* were made in Jamaica during the last opposition of the planet, in order to determine the Solar Parallax by means of the displacement of the planet in Right Ascension, when far east and far west of the meridian as seen at a single place.

The small Observatory is situate about $5\frac{3}{4}$ miles south-east of Montego Bay, on a range of hills 1800 feet above the sea-level. The buildings consist of a square Transit-room and a circular tower for the Equatoreal. The piers of the instruments are founded on solid rock, and the whole of the mason-work is compact and substantial.

The instruments were made by Messrs. Cooke & Sons, of York, and the mounting of the Equatoreal is very firm and massive. The aperture of the object-glass is only 4 inches, but the definition is very good. The transit eye-piece has five wires about $2'\,45''$ apart, and a power of 120 was employed. The Transit Instrument has an aperture of 3 inches. The clock is provided with a mercurial compensating pendulum and the dead-beat escapement; it was an inexpensive instrument (cost-

ing only £35), being in fact chiefly intended for adjusting the Equatoreal. The daily change of rate was generally small, $0^{s}\cdot5$ or thereabouts, but there was great irregularity of rate: for instance, an interval of 120 seconds between two stars is generally measured by $119^{sec}\cdot9$ and $120^{sec}\cdot1$ alternately; but often there is far greater irregularity, and differences of $0^{sec}\cdot2$, $0^{sec}\cdot3$, and $0^{sec}\cdot4$ occur between consecutive measures of such an interval.

The observations were commenced on the 4th of August, and were continued, with but few interruptions, up to September 17, when the weather broke up. The observed stars are numbered according to the list given in the *Monthly Notices*, November 1875, the only additional star employed being one of the $7\frac{1}{2}$ mag., referred to as *c* (see *Nature*, December 14, 1876). The work is divided into four sections: the first relating to the adjustment of the Equatoreal; the second to the reduction of the observations; the third containing the observations themselves; and the last the determination of the Solar Parallax in accordance with the previous sections.

By applying the weights as originally determined, the value obtained for the Solar Parallax was $8''\cdot80$. But, dividing the observations into two classes—the first class containing those which were made with two comparison stars, and the second class those made with only one comparison star—it was found that the mean value of $(\text{error} \times \text{weight})^2$ was about five times as great for the second class as for the first. Consequently the weight due to the quality of the first method is to the weight due to the quality of the second method as 5 : 1 nearly; but this result depending very much on the observations made on one night, it was thought advisable to reduce the ratio, and the ratio adopted was 4 : 1; and multiplying accordingly the former weights of the first class by 4, the resulting Solar Parallax was

$$8''\cdot789,$$

with a probable error of $0''\cdot060$.

Dun Echt Observatory Publications, Vol. II. Mauritius Expedition, 1874. Division I. Determination of the Solar Parallax by observations of the Minor Planet Juno (4) at Opposition, together with a description of the Heliometer used in the observations. Pp. i. to xiv. and 1 to 212.

It is proposed to give in another volume full reports of the chronometric and telegraphic determinations of the longitude of Mauritius, together with the observations for latitude and time which were made during Lord Lindsay's stay in the island; and

also to defer to a fourth volume the details of the observations of the Transit of *Venus* proper, comprising those made with the heliometer and the 6-inch Equatoreal and the accounts of the photographic arrangements and discussion of the negative obtained. The present volume relates to the determination of the Solar Parallax by the method described in the paper by Lord Lindsay and Mr. Gill (*Monthly Notices*, vol. xxxiv., p. 279). The method in its simplest form consists in measuring the position-angle and the distance of a minor planet relative to a star in the evening, and similarly in the morning. The difference of results, corrected for the motion of the planet, gives the data for computing the horizontal parallax. The total parallactic displacement is very considerably less than in the case of an opposition of *Mars* or a transit of *Venus*, but the authors believed that the great accuracy attainable in the heliometric measurement of the distance of two minute points of light would more than compensate for that defect.

They think that these observations have been justified by the result; and, as the best method for a rigid determination of the Solar Parallax is one of the most important as well as the most generally interesting questions presented to the astronomer, they thought it best to submit the present investigation as first fruits of the expedition.

It is distinctly to be understood that *the result arrived at is not presented as an example of the accuracy of which the method is capable, but is to be regarded merely as a first imperfect experiment, made under somewhat unfavourable and trying circumstances, from which conclusions may be drawn as to the capabilities of the method for the future.*

It was intended that the observations of *Juno* should commence on October 10, and the necessary stars of comparison were selected, and the Right Ascension and Declination of the middle point between star and planet, as well as the position-angle and distance of the star of comparison from the planet, were computed and published (*Monthly Notices*, vol. xxxiv., pp. 296–300) for each evening and morning between that date and November 30.

Unfortunately the yacht "Venus" with the instruments did not reach Port Louis till November 2, and it was November 5 before the heliometer could be unpacked at Belmont. On the 7th the instrument was completely erected and adjusted by the Sun and stars in daylight, and the adjustments, though tested frequently afterwards, were not again touched until the instrument was dismounted. Owing to cloudy weather, the first observations were made only on the night of the 10th, but under circumstances so unfavourable as to be nearly worthless. The first series of value was made on November 12, and observations were secured altogether on twelve evenings and eleven mornings between that date and November 30. Many of these were by no means so complete as desirable, as appearing from the recorded observations themselves and the notes attached to them. *Juno*

being past opposition (opposition was on November 5) the object was to secure measures as soon as possible after sunset.

In correcting the equations for errors in the tables of *Juno* it was found that there was a discordance between the Washington and Cambridge (U.S.) observations of the planet on the one side and the Greenwich observations on the other; and it was considered best to reduce the observations on both systems. This was accordingly done, and in the final equations between Δa, $\Delta\delta$, z, and $\Delta\pi$, the values of the constant term are given according to these two systems respectively. The assumed value of the Solar Parallax was $8''{\cdot}85$, and applying hereto the values of $\Delta\pi$ coming out from the equations, the resulting Mean Solar Parallax comes out—

1st System.	2nd System.
$8''{\cdot}77 \pm 0''{\cdot}041$.	$8''{\cdot}76 \pm 0''{\cdot}042$.

It is stated that the values of these probable errors depend upon too few observations to be trustworthy, and that the authors do not themselves attach high importance to the deduced value of the parallax. But that to which they do attach high importance is the very remarkable accuracy of the method as shown from the residuals when the value in question is substituted in the equations. From the short table given it appears that the resulting probable error for a complete observation is on the first system $\pm 0''{\cdot}073$, and on the second system $\pm 0''{\cdot}082$, confirming beyond doubt the conclusion arrived at in the paper in the *Monthly Notices*, that it is possible, by means of the heliometer, to determine the place of a minor planet relative to two stars (or, if within a distance of $1000''$, to one star) with a probable error of less than one-tenth of a second of arc.

Accounts have been received of the Lunar Eclipse of the 23rd of August 1877, as observed at Stonyhurst by the Rev. S. J. Perry, at Brighton by Mr. H. Pratt, at St. Leonards-on-Sea by Mr. J. E. H. Peyton, and at Morges, Switzerland, by Mr. V. Fasel.

Errata.

p. 40, line 1, for "Wills" read "With."

Vol. xxxiii. p. 100, line 9 from bottom, for $46°\ 30'\ 0''$ read $46°\ 30'\ 36''$.
" " 8 " " $6°\ 31'\ 0''$ " $6°\ 29'\ 0''$.
" " " " " $0^h\ 26^m\ 2^s$ " $0^h\ 25^m\ 56^s$.

Vol. xxxiv. p. 425, line 9 from bottom, for $46°\ 30'\ 0''$ read $46°\ 30'\ 36''$.
" " 8 " " $6°\ 31'\ 0''$ " $6°\ 29'\ 0''$.
" " " " " $0^h\ 26^m\ 2^s$ " $0^h\ 25^m\ 56^s$.
fast of Greenwich.